THE JOURNAL OF PHILOSOPHY

VOL. LVII, NO. 11: MAY 26, 1960

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PUBLISHED FORTNIGHTLY

BY THE

JOURNAL OF PHILOSOPHY, INC.

THE JOURNAL OF PHILOSOPHY

FOUNDED BY FREDERICK J. E. WOODBRIDGE AND WENDELL T. BUSH

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Communications to the editors should be addressed to the Managing Editor, 713 Philosophy Hall, Columbia University, New York 27, N. Y. Subscriptions and advertisements should be sent to THE JOURNAL OF PHILOSOPHY, 713 Philosophy Hall, Columbia University, New York 27, N. Y. The annual subscription price (26 numbers) is $6.50; to students, $5.25. The price of single numbers is $0.50; double numbers, $1.00; complete volumes, $7.00.

THE JOURNAL OF PHILOSOPHY is owned by the Journal of Philosophy, Inc., and published fortnightly at Prince and Lemon Sts., Lancaster, Pa. Editorial Office: 713 Philosophy Hall, Columbia University, New York 27, N. Y. President, Albert G. Redpath; Vice President, John H. Randall, Jr.; Treasurer, Frederick A. Dewey; Secretary, Corliss Lamont.

Second-class postage paid at Lancaster, Pa.

Stechert-Hafner, Inc., New York - Atlas Publ. & Distr. Co., Ltd., London - Hermann & Cie., Paris - Nicola Zanichelli, Bologna - H. Bouvier u. Co., Bonn a/Rh. - Libreria Franco Stuyck, Madrid - F. Machado & Cia, Porto - Gerold & Co., Wien - Librairie Payot, S. A., Lausanne - The Maruzen Company, Tokyo.

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VOLUME LVII, No. 11 MAY 26, 1960

THE JOURNAL OF PHILOSOPHY

MUST THE SCIENTIST MAKE VALUE JUDGMENTS?*

The scientific man has above all things to strive at self-elimination in his judgments, to provide an argument which is true for each individual mind as for his own.

KARL PEARSON

TWO assumptions implicit in Pearson's characterization of "the scientific man" have been called into question in recent years: (*a*) At least one major goal of the scientist *qua* scientist is to make judgments—i.e., to accept or reject hypotheses—and to justify his judgments. (*b*) The scientific inquirer is prohibited by the canons of scientific inference from taking his attitudes, preferences, temperament, and values into account when assessing the correctness of his inferences.

One currently held view affirms (*a*) but denies (*b*). This position maintains that the scientist does and, indeed, must make value judgments when choosing between hypotheses. The other position upholds the value-neutrality thesis (*b*) at the expense of the claim that scientific inference issues in the acceptance and rejection of hypotheses (*a*). According to this view, a scientific inquiry does not terminate with the replacement of doubt by belief but with the assignment of probabilities or degrees of confirmation to hypotheses relative to the available evidence.

In this paper, a critical examination of these conflicting conceptions of scientific inference will be undertaken; the *prima facie* tenability of the claim that scientists can, do, and ought to accept and reject hypotheses in accordance with the value-neutrality thesis will be defended; and some indication will be given of the kind of question that must be answered before this plausible view can be converted into a coherent and adequate theory of the relation of values to scientific inference.

* I wish to acknowledge my debt to Sidney Morgenbesser, whose critical comments in conversation have greatly influenced my thinking on this question, and to Mortimer Kadish and John McLellan, whose reactions to earlier drafts of this paper have helped shape the final result.

I

The tenability of the value-neutrality thesis has been questioned by C. W. Churchman[1] and R. B. Braithwaite,[2] at least insofar as it applies to statistical inference. However, the most explicit and sweeping attack against the value-neutrality thesis is to be found in an article by Richard Rudner, who argues that the scientist must make value judgments in drawing any kind of non-deductive inference.[3]

> Now I take it that no analysis of what constitutes the method of science would be satisfactory unless it comprised some assertion to the effect that the scientist as scientist accepts or rejects hypotheses.
>
> But if this is so then clearly the scientist as scientist does make value judgments. For, since no scientific hypothesis is ever completely verified, in accepting a hypothesis the scientist must make the decision that the evidence is *sufficiently* strong or that the probability is *sufficiently* high to warrant the acceptance of the hypothesis. Obviously our decision regarding the evidence and respecting how strong is "strong enough" is going to be a function of the *importance*, in the typically ethical sense, of making a mistake in accepting or rejecting the hypothesis. Thus, to take a crude but easily manageable example, if the hypothesis under consideration were to the effect that a toxic ingredient of a drug was not present in lethal quantity, we would require a relatively high degree of confirmation or confidence before accepting the hypothesis—for the consequences of making a mistake here are exceedingly grave by our moral standards. On the other hand, if, say, our hypothesis stated that, on the basis of a sample, a certain lot of machine stamped belt buckles was not defective, the degree of confidence we should require would be relatively not so high. *How sure we need to be before we accept a hypothesis will depend on how serious a mistake would be.*[4]

Rudner's claim is not that values play a role in the scientist's selection of research problems, nor is he arguing that scientists often let their attitudes, values, and temperaments influence their conclusions. These points are relevant to the psychology and sociology of inquiry but not to its logic. Rudner is making an assertion about the requirements imposed upon the inquirer who embraces the goals and the canons of scientific inference.[5] He contends that the scientist in his capacity as a scientist *must* make

[1] C. W. Churchman, *Theory of Experimental Inference* (New York: Macmillan, 1948), Ch. XV.

[2] R. B. Braithwaite, *Scientific Explanation* (Cambridge: Cambridge University Press, 1955), pp. 250–254.

[3] R. Rudner, "The Scientist *qua* Scientist Makes Value Judgments," *Philosophy of Science*, Vol. XX (1953), p. 3.

[4] *Ibid.*, p. 2.

[5] The canons of scientific inference can be construed to be normative principles. The value-neutrality thesis does not deny this but does insist that given an initial commitment to these principles, the scientist need not and should not let his values, attitudes, and temperament influence his inferences any further. It is this claim that Rudner appears to deny.

value judgments even if it is psychologically possible for him to avoid doing so. His argument for this conclusion can be summarized in the following series of statements:

(1) The scientist *qua* scientist accepts or rejects hypotheses.

(2) No amount of evidence ever completely confirms or disconfirms any (empirical) hypothesis but only renders it more or less probable.

(3) As a consequence of (1) and (2), the scientist must decide how high the probability of a hypothesis relative to the evidence must be before he is warranted in accepting it.

(4) The decision required in (3) is a function of how important it will be if a mistake is made in accepting or rejecting a hypothesis.

The need for assigning minimum probabilities for accepting and rejecting hypotheses (3) is a deductive consequence of the claim that scientists accept and reject hypotheses (1) and the corrigibility of empirical hypotheses (2). Since (2) is a cardinal tenet of an empiricist philosophy of science and will not be questioned in this paper, the first part of Rudner's argument reduces to the correct claim that if (1) is true (3) is true.

Rudner's rejection of the value-neutrality thesis cannot be justified, however, on the basis of (3) alone. He must show that the assignment of minimum probabilities is a function of the importance of making mistakes (4). But (4) cannot be obtained from (3) without further argument.[6] Rudner attempts to fill the gap by citing illustrations from quality control and appealing to current theories of statistical inference.[7] He believes that the problem of choosing how to act in the face of uncertainty, which is the fundamental problem of quality control, is typical of all scientific inquiry and concludes from this that the importance of making mistakes must be taken into account in all scientific inference.

This argument seems to rest upon certain assumptions adopted more or less explicitly by Rudner and Churchman.[8] These as-

[6] Actually Rudner's version of (4) is stronger than mine. According to Rudner, the importance of making a mistake can be construed in "a typically ethical sense." In order to simplify the discussion, this rider will be dropped. The importance of making a mistake will be understood to be a function of the values, attitudes, preferences, and temperament of the investigator or group whose interests he serves regardless of the ethical character of these values, etc. Understood in this sense, (4) is still incompatible with the value-neutrality thesis.

[7] Rudner, *op. cit.* pp. 2–3.

[8] C. W. Churchman, "Science and Decision Making," *Philosophy of Science*, Vol. XXXIII (1956), p. 248.

sumptions involve the notion of acting on the basis of a hypothesis relative to an objective. To say "X acts on the basis of H relative to some objective P" is to assert that X carries out action A where A is the best procedure[9] to follow relative to P, given that H is true. The Rudner-Churchman assumptions can now be stated as follows:

(5) To choose to accept a hypothesis H as true (or to believe that H is true) is equivalent to choosing to act on the basis of H relative to some specific objective P.

(6) The degree of confirmation that a hypothesis H must have before one is warranted in choosing to act on the basis of H relative to an objective P is a function of the seriousness of the error relative to P resulting from basing the action on the wrong hypothesis.

Assumption (6) is a version of a principle adopted by Pearson, Neyman, and Wald in their theories of statistical inference. The plausibility of Rudner's argument from quality control (where the problem is how to act on the basis of hypotheses) to (4) is due largely to the reasonableness of this presupposition. However, (6) without (5) will not yield (4).

Unlike (6), (5) cannot be justified by an appeal to the authority of the statisticians. Not only are these authorities fallible, but some of them have been non-committal regarding the acceptability of (5).[10] Substantial grounds can be offered for praising this exercise of caution.

II

An interesting case against the tenability of (5) has been made by Richard Jeffrey. Jeffrey considers the problem of deciding whether a given batch of polio vaccine is free from active

[9] Perhaps "A is believed by X to be the best procedure" should replace "A is the best procedure." The following discussion does not, however, demand a choice between these two definitions.

[10] "The terms 'accepting' and 'rejecting' a statistical hypothesis are very convenient and are well established. It is important, however, to keep their exact meaning in mind and to discard various additional implications which may be suggested by intuition. Thus, to accept a hypothesis H means only to decide to take action A rather than action B. This does not mean that we necessarily believe that the hypothesis H is true. Also if the application of a rule of inductive behavior 'rejects' H, this means only that the rule prescribes action B and does not imply that we believe that H is false." (J. Neyman, *A First Course in Probability*, New York, H. Holt & Co., 1950, pp. 259–260.) In this passage, Neyman does identify accepting a hypothesis H with acting on H. However, he refuses to identify accepting H with believing that H. In effect, therefore, he suspends judgment regarding the truth of (5).

polio virus. The seriousness of the consequences of mistakenly accepting the hypothesis would seem to demand that we confirm the hypothesis to a far higher degree before accepting it than would be the case if we were interested in the quality of a batch of roller skate bearings.

But what determines these consequences? There is nothing in the hypothesis, "This vaccine is free from active polio virus," to tell us what the vaccine is *for*, or what would happen if the statement were accepted when false. One naturally assumes that the vaccine is intended for inoculating children, but for all we know from the hypothesis it might be intended for inoculating pet monkeys. One's confidence in the hypothesis might well be high enough to warrant inoculation of monkeys but not of children.[11]

Jeffrey's point can be reformulated as follows: Action on the basis of a hypothesis H is always relative to an objective P. Consequently if accepting H is identical with acting on the basis of H (5), accepting H in an "open-ended" situation [12] where there is no specific objective is impossible. But accepting H is possible in open-ended situations, for it is compatible with different and even conflicting objectives. Hence, (5) must be rejected.

In a reply to Jeffrey's paper, Churchman compares Jeffrey's open-ended decision problems to situations that occur in production. Suppose that a manufacturer wishes to place on the market a certain product (rope) that has many different uses. Churchman points out that procedures are available to the manufacturer in terms of which he can single out needs that his product should be designed to meet. He contends that similar procedures must be employed if we are to accept and reject hypotheses intelligibly.

In this sense, it is certainly meaningless to talk of *the* acceptance of the hypothesis about the freedom of a vaccine from active polio virus, provided the information has a number of different uses. Even within one business organization one can readily point out that the many uses of information imply many different criteria for the "acceptance" or "rejection" of hypotheses.[13]

Churchman's argument seems to be this: *If* "accepting a hypothesis H" is understood in a sense that makes (5) true, then open-ended decision problems involving the acceptance or rejection of hypotheses can be treated like open-ended production problems. The solvability and, hence, the intelligibility of such problems requires the elimination of the open-endedness.

This true observation does not meet, however, the major point of Jeffrey's objection. Jeffrey's argument attempts to show that

[11] R. C. Jeffrey, "Valuation and Acceptance of Scientific Hypotheses," *Philosophy of Science*, Vol. XXXIII (1956), p. 242.

[12] This expression is due to Churchman (*loc. cit.*).

[13] *Ibid.*, pp. 248–249.

in *one* sense of "accepting a hypothesis" to accept a hypothesis in an open-ended situation is perfectly meaningful and consistent. Consequently, in *that* sense, (5) does not hold.

An easy but cheap victory might be gained at Jeffrey's expense by pointing out that wherever a scientist does not appear to have an objective in mind, nonetheless, one can always be specified—namely, the objective of accepting true answers to questions as true. Accepting a hypothesis H would then be equivalent to acting on the basis of H relative to that objective.

Resorting to this strategy would be to miss the point of the discussion. To say that accepting a hypothesis is the same as acting on the basis of H in order to obtain true answers is tantamount to asserting that accepting H is equivalent to accepting H. One could not conclude from this alone that the problem of deciding what to believe is on all fours with decision problems in quality control—at least with respect to the value-neutrality thesis. In the latter kind of problem, the objectives are "practical"; in the former, they are "theoretical."

In order to avoid misunderstanding, therefore, an open-ended decision problem will be understood to be a decision problem for which no practical objective has been specified.[14] Consequently, the issue at stake in the debate between Jeffrey and Churchman is whether there is any sense in which a person can meaningfully and consistently be said to accept a hypothesis as true without having a practical objective. The following considerations are offered in favor of an affirmative answer to this question.

(i) Many apparently intelligible questions are raised and answered in the sciences for which practical objectives are difficult to specify. What practical objectives are at stake when an investigator is deciding whether to accept or reject the principle of parity, the hypothesis of an expanding universe, or the claim that Galileo never conducted the Leaning Tower experiment? One could try to show that appearances are deceiving and that practical objectives are always the goals of such decision problems. However, this would be difficult to prove. Furthermore, it would not follow that appearances *must* be deceiving and that practical

[14] By a "theoretical" objective, I shall understand any objective that is concerned with selecting true hypotheses from a given list. A practical objective is one that is not theoretical. This dichotomy overlooks distinctions between ethical, practical, and aesthetic objectives by grouping them together. It also treats many objectives as practical that might legitimately be held to be theoretical. The purpose of the twofold partition of objectives, however, is to avoid a trivial interpretation of (5) while permitting Churchman and Rudner as much leeway as possible in their interpretation of this assumption.

objectives *must* be operative. Indeed, the cases just cited would normally be considered to be problems of deciding what to accept as or believe to be true regardless of whether practical objectives are involved. This seems to indicate that there is a sense of "accepting a hypothesis" which is meaningfully applied to choices in open-ended situations.

(ii) Even in the case of decision problems where practical objectives are involved, it often seems appropriate to distinguish between acting on the basis of a hypothesis relative to that objective and accepting the hypothesis as true. Suppose that an investor in oil stocks knows that if a certain oil company whose stocks are selling at a low price strikes oil at a certain location, the price of the stock will increase one hundredfold. The investor might buy stock in the company while suspending judgment as to the eventual discovery of oil. Here is a case where one would normally say that a person has acted on the basis of a hypothesis and perhaps was justified in doing so without accepting the hypothesis as true or being warranted in so accepting it.

One could reply by saying that the investor refused to accept the hypothesis because such acceptance would have been tantamount to acting on the basis of the hypothesis of an oil strike relative to some practical objective other than making a profitable investment. However, such an objective would not always be easy to find. Furthermore, the situation would normally be considered a case of action without belief regardless of whether the existence of a practical objective could be shown or not.

(iii) There seems to be a sense in which it is possible for a person to believe in the truth of a hypothesis and nonetheless refuse to act on it. He may even be justified in proceeding in this fashion. The Sabin live virus polio vaccine serves as an illustration. The available evidence might warrant belief in the safety and effectiveness of the vaccine without justifying a program of mass inoculation.[15]

[15] This claim might seem counterintuitive. There is a widely held view that if a person really believes in a hypothesis he should be ready to act on it. This "put up or shut up" analysis may be understood in two ways: (*a*) belief in H implies acting on the basis of H, and (*b*) belief in H implies that one ought to act on the basis of H. R. M. Martin seems to adopt the former view (*Toward a Systematic Pragmatics*, Amsterdam, North Holland, 1959, p. 11). This version of the "put up or shut up" analysis does not seem adequate to at least one familiar sense of "accepting a hypothesis." The very fact that people often think that one ought to act on a hypothesis if one believes it implies that one might not so act. Sense (*b*) of the "put up or shut up" analysis seems more plausible. Nonetheless it yields results that themselves appear to be counterintuitive. If this thesis demands readiness

(iv) A plausible case can be made for saying that even when a person is deciding how to act in order to realize a practical objective he will have to accept some statements as true in a sense that does not meet the conditions of (5). The evidence upon which he bases his decisions consists of statements which he accepts as true. He might have to accept the truth of statements asserting the degrees to which various hypotheses are confirmed relative to the available evidence. Finally, he will also have to accept the truth of statements that indicate the best actions relative to his objectives given the truth of various hypotheses.[16]

The considerations just advanced suggest that there is a familiar sense in which a person can meaningfully and consistently accept or reject a hypothesis in an open-ended situation. In that sense, (5) is false and Rudner's argument in favor of (4) and against the value-neutrality thesis fails.

This result need not in itself be fatal to the Churchman-Rudner position. Apologists for this view could admit the meaningfulness of this sense of "accepting a hypothesis" and deny that the aim of the sciences is (or ought to be) to accept or reject hypotheses in that sense. They might contend that scientific inferences indicate how one ought to act on the basis of hypotheses but not what one ought to believe. The rejection of the value-neutrality thesis would flow quite naturally from this transmutation of scientific inquiry into a quest for normative principles. Oddly enough, however, it is Jeffrey, an apparent defender of the value-neutrality thesis, who denies that scientists accept and reject hypotheses.

III

Jeffrey proposes a conception of the aim and function of science also suggested by Carnap[17] and Hempel.[18] According to this view, a scientist does not, or at least should not, accept and reject hypotheses. Instead, he should content himself with assigning

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to act relative to *any* objective, then one would not be warranted in accepting a hypothesis as true unless the degree of confirmation approached certainty. For there is always the possibility that some objectives exist relative to which mistakes are so serious as to demand enormously high degrees of confirmation. Such a requirement seems unreasonable. On the other hand, if the objectives relative to which one should be ready to act are restricted in some way, it is difficult to see how the restrictions could be specified without destroying the initial plausibility of the "put up or shut up" analysis.

[16] I owe this observation to Mortimer Kadish.

[17] R. Carnap, *Logical Foundations of Probability* (Chicago: University of Chicago Press, 1950), pp. 205–207.

[18] C. G. Hempel, review of Churchman's *Theory of Experimental Inference*, in *Journal of Philosophy*, Vol. XLVI (1949), p. 560.

degrees of confirmation to hypotheses relative to the available evidence. Anyone who is confronted with a practical decision problem can go to the scientist to ascertain the degrees of confirmation of the relevant hypotheses. He can then utilize this information together with his own estimates of the seriousness of mistakes in order to decide upon a course of action.

One consequence of this view is that all non-deductive inference in science consists in assigning degrees of confirmation to hypotheses relative to given evidence. Indeed, Carnap defines inductive inference in this way.[19] Hence, if Carnap is correct in maintaining that degrees of confirmation can be ascertained without consideration of values, the Carnap-Hempel-Jeffrey view supports the value-neutrality thesis.[20] However, the value neutrality thesis is upheld at the expense of the claim that scientists accept or reject hypotheses. In this respect, the Carnap-Hempel-Jeffrey view breaks as radically with tradition as does the Braithwaite-Churchman-Rudner position.

In his paper, Jeffrey offers an extremely clever argument to show that scientists can neither accept nor reject hypotheses.

> On the Churchman-Braithwaite-Rudner view it is the task of the scientist as such to accept and reject hypotheses in such a way as to maximize the expectation of good for, say a community for which he is acting. On the other hand, our conclusion is that if the scientist is to maximize good he should refrain from accepting or rejecting hypotheses, since he cannot possibly do so in such a way as to optimize every decision which may be made on the basis of those hypotheses. We note that this difficulty cannot be avoided by making acceptance relative to the most stringent possible set of utilities (even if there were some way of determining what that is) because then the choice would be wrong for all less stringent sets. One cannot, by accepting or rejecting the hypothesis about the polio vaccine, do justice both to the problem of the physician and the veterinarian. The conflict can be resolved if the scientist either contents himself with providing them both with a single probability for the hypothesis (whereupon each makes his own decision based on the utilities peculiar to his problem) or if the scientist takes on the job of making a separate decision as to the acceptability of the hypothesis in each case. In any event, we conclude that it is not the business of the scientist as such, least of all of the scientist who works with lawlike hypotheses, to accept or reject hypotheses.[21]

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[19] Carnap, *op. cit.*, p. 206.

[20] The difference between this view and the revised version of the Churchman-Rudner position suggested above is that the latter considers the scientist as a formulator of practical policy whereas the former considers him to be an adviser to the policy maker. This difference reflects itself in differing conceptions of non-deductive inference. According to the revised Churchman-Rudner view, the "conclusion" of a non-deductive inference is a choice of a course of action. According to the Carnap-Hempel-Jeffrey view, the conclusion is an assignment of a degree of confirmation to a hypothesis.

[21] Jeffrey, *op. cit.*, p. 245.

Jeffrey's argument rests upon two lemmas: (*a*) if scientists accept and reject hypotheses (1), then they must make value judgments (4); and (*b*) if (1) is true, then (4) is false. The inevitable conclusion is that (1) is false—i.e., that the scientist neither accepts nor rejects hypotheses.

Jeffrey accepts (*a*) without any question as having been established by Rudner. His argument for (*b*) may be paraphrased as follows: Deciding whether to accept or reject a hypothesis is an open-ended decision problem—i.e., there is no practical objective in terms of which seriousness of error can be assessed. Hence, if a scientist decides to accept or reject a hypothesis, he cannot be taking the seriousness of error into account. Consequently, if (1) is true, (4) is false.

In spite of its persuasive character, Jeffrey's argument breaks down at several points.

(i) Rudner's argument for lemma (*a*) has already been shown to hold only if accepting a hypothesis is understood to be meaning*less* in open-ended situations. On the other hand, Jeffrey's argument for (*b*) depends upon the understanding that accepting a hypothesis is meaning*ful* in such cases. Hence, Jeffrey is guilty of equivocation.

(ii) Jeffrey's argument from the truth of (1) to the falsity of (4) depends upon the assumption that the decision problem is an open-ended one. An open-ended decision problem has been understood to be one that lacks a *practical* objective. However, such problems may still have a theoretical objective. It is at least an open question whether such an objective can serve as a basis for ascertaining the seriousness of mistakes.

(iii) Even if theoretical objectives cannot function in this way, Jeffrey's inference from (1) to the negation of (4) can still be avoided. It has been argued that Jeffrey is correct in asserting and Churchman is wrong in denying that there is a sense of "accepting a hypothesis" that is meaningful in open-ended situations. This does not mean, however, that this sense of "accepting a hypothesis" is meaningful *only* in open-ended situations. A person may decide what to believe only in order to believe true statements. But he may wish to believe statements which are true and which have some other desirable characteristic such as simplicity, explanatory power, effectiveness as propaganda, or a consoling emotive connotation. And the sense in which he accepts a statement as true in attempting to realize one of these objectives will be the same sense in which he might accept statements as true in open-ended situations. Again, it is at least an open question

whether a scientist *qua* scientist has such a practical objective in accepting and rejecting hypotheses and, hence, has a basis for determining the seriousness of mistakes.

The failure of Jeffrey's argument does not, of course, imply the falsity of his conclusion. Indeed, another argument can be offered for rejecting (1). Whatever may be the merits of the inference from (1) to (4), empiricists are committed to accepting the inference from (1) to (3)—i.e., the inference from the claim that scientists accept and reject hypotheses to the need for assigning minimum probabilities for such acceptance and rejection. How are such minimum probabilities to be assigned? If no plausible alternative to a procedure that takes the values of the investigator into account is available, then (1) entails the rejection of the value-neutrality thesis.

Defenders of the Carnap-Hempel-Jeffrey view might feel that we are in such a predicament. Not wishing to abandon the value-neutrality thesis, they reject the conception of the scientist as one who accepts and rejects hypotheses.[22] However, following this strategy is like crashing into Scylla in order to avoid sinking in Charybdis. As Jeffrey himself admits,[23] the scientific literature suggests that scientists do often accept and reject hypotheses in a sense incompatible with (5). Furthermore, they often appear to feel that it is at least part of their business to do so. Consequently, an attempt to construct a theory of scientific inference based on the assumption that scientists do accept and reject hypotheses seems warranted.

IV

The question that remains is whether on this assumption the value-neutrality thesis can be maintained. An answer to this question seems to depend upon determining the manner in which minimum probabilities for accepting and rejecting hypotheses are assigned according to the canons of scientific inference. A study of the procedures for assigning minimum probabilities cannot be undertaken in this paper. Nonetheless, two possible outcomes of such an investigation that would support the value-neutrality thesis are worth mentioning. A consideration of these possibilities will serve to clarify the content of the value-neutrality thesis and to focus attention on the issues that must be settled before an adequate assessment of its merits can be made.

[22] Hempel (*loc. cit.*) comes closer to arguing in this way than either Carnap or Jeffrey.

[23] Jeffrey, *op. cit.*, p. 246.

(A) The necessity of assigning minimum probabilities for accepting or rejecting hypotheses does not imply that the values, preferences, temperament, etc. of the investigator, or of the group whose interests he serves, determine the assignment of these minima. The minimum probabilities might be functions of syntactical or semantical features of the hypotheses themselves. Indeed, they might not be determined by any identifiable factors at all other than certain rules contained in the canons of inference. These rules might fix the minima in such a way that given the available evidence two different investigators would not be warranted in making different choices among a set of competing hypotheses. If the canons of inference did work in this way, they would embody the value-neutrality thesis.

(B) Even if the minimum probabilities were functions of identifiable values, the value-neutrality thesis would not necessarily have to be abandoned. When a scientist commits himself to certain "scientific" standards of inference, he does, in a sense, commit himself to certain normative principles. He is obligated to accept the validity of certain types of inference and to deny the validity of others. *The values that determine minimum probabilities may be part of this commitment.* In other words, the canons of inference might require of each scientist *qua* scientist that he have the same attitudes, assign the same utilities, or take each mistake with the same degree of seriousness as every other scientist. The canons of inference would, under these circumstances, be subject to the value-neutrality thesis; for the value-neutrality thesis does not maintain that the scientist *qua* scientist makes no value judgments but that given his commitment to the canons of inference he need make no further value judgments in order to decide which hypotheses to accept and which to reject.[24]

Thus, the tenability of the value-neutrality thesis does not depend upon whether minimum probabilities for accepting or rejecting hypotheses are a function of values but upon whether the canons of inference require of each scientist that he assign the same minima as every other scientist.

V

The arguments offered in this paper do not conclusively refute the major theses advanced by Rudner or Jeffrey. However, these arguments have justified further examination of the view that

[24] It should also be clear that the value-neutrality thesis says nothing concerning the rationale for adopting scientific canons of inference but only about the content of these canons.

scientists accept or reject hypotheses in accordance with the value-neutrality thesis. In particular, it has been shown that Rudner and Churchman have failed to prove that a scientist must take the seriousness of mistakes into account in order to accept or reject hypotheses where the seriousness of mistakes is relative to practical objectives; it has also been shown that even if Rudner and Churchman were correct, the value-neutrality thesis would not entail Jeffrey's abandonment of the view that scientists accept or reject hypotheses; and, finally, it has been argued that even if scientists must take the seriousness of mistakes or other values into account in determining minimum probabilities, they may still accept or reject hypotheses in accordance with the value-neutrality thesis.

The outcome of this discussion is that the tenability of the value-neutrality thesis depends upon whether the canons of scientific inference dictate assignments of minimum probabilities in such a way as to permit no differences in the assignments made by different investigators to the same set of alternative hypotheses. An answer to this question can only be obtained by a closer examination of the manner in which minimum probabilities are assigned in the sciences. This problem will be the subject of another paper.

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COULD TIME FLOW? IF SO, HOW FAST?

PEOPLE have said, and will say, that time flows. Some say that it flows always at the same rate,[1] others that it flows faster on some occasions and slower on others. But certain philosophers dislike this kind of talk. They argue that time does not flow and cannot be said to flow in any literal sense. They base their view on a very simple argument which they take to be virtually conclusive, and they go on to discuss the metaphorical character of statements about the flowing of time. I think I can show that this argument is not conclusive, and that statements about the flowing of time, and the rate of flow of time, can, and probably usually do, have a literal, defensible, and easily understandable meaning. About the metaphorical character of such statements I shall say nothing.

[1] Perhaps Newton's statement that time "flows equably" can be interpreted to mean that time always flows at the same rate.

The argument in question is stated as clearly as could be desired by Professor Black, who says, "If the claim that time always flows had a literal sense, we ought to be entitled to ask how fast time is flowing. And if so, there would have to be a supertime for measuring the rate of flow of ordinary time. With regard to that super-time, a similar question would immediately arise, viz., how fast *it* was flowing; and this would imply still another time for its answer. And so on without end."[2] He concludes that to accept such an infinite regress is to compound confusion, and he says that he cannot see that any sense attaches to the question, "How fast is time flowing?" It seems to me, however, aside from the obvious point that there is no need for anyone who thinks that "ordinary time" flows to say that "super-time" also flows, that this argument has serious defects which stem from an insufficient analysis of the notion of measuring time. In order to reveal these defects, I shall present a series of points concerning the measurement of time, which I take to be correct, but which I shall not here defend.

1. Time is measured by clocks. A clock may be broadly considered to be any device which displays a recurring, identifiable pattern.

2. Time is measured in arbitrarily chosen standard units, and their multiples and fractions. It is the recurrence of the identifiable pattern of a clock which marks the repetition of the standard unit of time, or multiple or fraction thereof.

3. If a given clock is chosen as the standard clock which establishes a basic standard unit of time, *there is no way whatsoever* of determining if the repeated standard unit of time is always the same "length." In fact, whatever may be the case with standard units of space, there is no literal or factual meaning to supposing, if a solar day, for example, is chosen as the basic standard unit of time, either that one solar day is the same "length" as another solar day, or that it is a different "length" from another.[3] The fact that the repetition of the recurring

[2] Max Black, "The 'Direction' of Time," *Analysis*, Vol. 19, No. 3, Jan. 1959, pp. 56–57. For a similar argument, see also: Donald C. Williams, "The Sea Fight Tomorrow," in Henle (ed.), *Structure, Method and Meaning*, p. 304.

[3] I hope that this statement will not be construed as an attempt to legislate concerning meaning. With respect to meaning, the only point I should wish to make is that no matter what types or kinds of meaning are recognized, it is useful to distinguish what may be called literal or factual meaning from other kinds. Although it is difficult to state precisely what

pattern of a clock may be correlated with a calibrating device which may display an arbitrary number of equal divisions, does *not* establish that the "length" of the basic unit of time, or the "length" of the divisions of it indicated by the calibrating device, is always the same. Insofar as the expression "length of time" has a literal or factual meaning, this meaning derives from the *number* of units of time, not from the supposed "length" of the units themselves.

4. There is no literal or factual meaning to the expression "rate of flow" or "rate of change" except that which derives from the ratio comprised by the *number* of repeated units of one measurable "thing" over the *number* of repeated units of another measurable "thing." Wherever these numbers are not, at least in principle, determinable, the expression "rate of flow" or "rate of change" is not applicable.

5. It makes literal sense to say that one clock runs *faster* than another clock. Thus, if two clocks begin to display their recurring patterns simultaneously, one clock may tick off *more* units marked by its recurring pattern than does the other, before one of them ceases to function. It is possible to observe, for purposes of comparison, *what may count as* the simultaneous beginning of two clocks, and *what may count as* the end of the function of a clock. If this were not possible, stop-watches would be useless. To say that one clock runs faster than another means, in a literal sense, nothing more than that one clock ticks off more units than the other, counting from simultaneous starting to simultaneous stopping.

6. There is no "absolute" clock. Anything which displays the required recurring identifiable pattern *is* a clock, and could be used to establish a standard unit of time.

From the above, I think we can conclude that if one clock is chosen as a standard, and another faster clock is chosen as an alternative standard, then we can establish the ratio of the number of units ticked off by one clock over the number of units ticked off by

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criteria will serve to distinguish a literal or factual statement from others, it seems to me undeniable that some statements clearly are literal or factual in meaning and others clearly are not. And I should say that part of our clarity in this regard stems from our understanding, in the case of a literal or factual statement, what it would be like in our experience for the statement to be true, and what it would be like for it to be false. It seems to me that in the case of statements concerning a variation in "length" of a standard unit of time, we do not have such an understanding.

the other, counting from an arbitrarily chosen simultaneous starting to an arbitrarily chosen simultaneous stopping, and that this ratio would represent the rate of change of the time of one clock relative to the time of the other. Thus, the time of one clock could be said to flow, and, indeed, to flow with a finite measurable rate of change. Since there is no such thing as an absolute clock, the time measured by *any* clock could be said to flow relative to the time of some other clock. It should be borne in mind that the establishment of this ratio would *not* provide a means for verifying that the "lengths" of the units marked by either clock were either different or the same. It is a gratuitous and, literally speaking, *meaningless* assumption to suppose that two clocks which start simultaneously and stop simultaneously and tick off *different* numbers of units of time, do so in the "same time." The literal or factual meaning of the length of time measured by either clock consists solely in the numbers (but not merely integers) of the basic units actually ticked off, not in the "lengths" of the units, or of fractions thereof. Since it is literally or factually meaningless to say that the "length of time" from starting to stopping is the same for each clock, one must conclude that if one clock ticks off more units of time, it ticks off more time, and hence relative to the other clock is faster by a definite rate of flow.

Accordingly, we can now see what is wrong with the argument that time cannot literally be said to flow. Those who employ this argument apparently consider it a crushing and unanswerable question to ask how fast time flows. But it is just because this question *is* answerable that one must conclude that the time of some particular clock can be said to flow. It is just because it does make literal sense to say that time flows at a definite rate of change that one can say that time flows. It is not too difficult, moreover, to imagine the sort of thing which must obtain to enable a person to say that time flows, or to say that it passes quickly or slowly. Everyone carries around with him a number of clocks, which display recurring identifiable patterns, for example, heartbeat, breathing, hunger pangs, etc. Undoubtedly, there can be established a ratio between the number of recurrences of any one of these clocks and the number of recurrences of seconds, minutes, or hours of ordinary clocks, and it is not unlikely that, occasionally, individuals may be conscious of the approximate value of some such ratio. Thus, when anyone says that the minutes just *dragged* by slowly, it is quite possible that he means that the rate of change of the time of ordinary clocks relative to the time of some clock such as breathing was slow. It is not necessary to suppose that

such a person would have to count his respiration, for it is possible to measure things, although inaccurately, simply by guessing. With a little practice it is possible to guess lengths with *some* accuracy, and sometimes with surprising accuracy. Furthermore, it is well known that people can guess speeds and pronounce them either fast or slow, very fast or very slow, without having to express their guesses in terms of exact ratios.

The point which I wish to make, however, can be expressed very simply. If it is possible to say that a person's heartbeat is fast or slow in terms of seconds, it must be equally possible to say that seconds are fast or slow in terms of heartbeats. Only those who, for some reason, think that ordinary clocks determine sacrosanct and absolute standard units of time could deny this. But it may be that such physical and dependable clocks as breathing and heartbeat are much more important for our ordinary understanding of time than are calibrated clocks.

Perhaps at this point it will be objected that a human heart is *not* a dependable clock, and that scientists have ways of *verifying* that the lengths of time marked by human heartbeats vary widely compared with the uniform lengths of time ticked off by scientifically constructed clocks. This objection is easily answered. If, as I have argued, the sole means we have of determining a length of time is by determining the *number* of recurrences of units of time, then the notion of a length of time is always relative to some particular clock, and a comparison of the recurrences of two clocks could never be a means of verifying that the "lengths" of the units ticked off by one clock are either shorter or longer than those of the other. In order to verify that the units of time ticked off by a particular clock varied in "length," it would be necessary to compare the performance of this clock with that of a standard clock, the units of which did *not* vary in "length." But it is impossible to verify that the units of time ticked off by any such standard clock are always the same "length," for we cannot compare the "length" of one of its units of time with the "length" of another of its units.

This objection raises the question of what is involved in the correction of time-pieces. Ordinarily, we think it is possible to correct a clock by making adjustments which rectify its performance in comparison with another clock which is known to be "correct" or "more correct." Furthermore, it is well known that scientists employ methods of making corrections for a variation in the performance of a standard clock, such as that comprised by the rotation of the earth. There is nothing in these procedures,

properly understood, with which I should wish to quarrel. Certainly, no one could deny that time-pieces frequently are, in some sense, "corrected." In my view, however, these procedures do not establish anything concerning the "length" of a standard unit of time.

There are two factors involved in the correction of time-pieces, viz., synchronization and regularity. If a clock is synchronized with a standard clock, it can be said to be correct in the sense that it does not deviate from the standard. And if a standard clock is free from irregularity, it can be said to be correct in that sense. If a certain clock is chosen as a standard clock, it is desirable that other clocks used for the same purposes as the standard clock should be synchronized with it so that they "keep the same time." Two clocks may be said to be exactly synchronized if they always tick off exactly the same number of units of time, counting from arbitrarily chosen simultaneous startings to arbitrarily chosen simultaneous stoppings. Even though it is doubtful whether two clocks can ever be exactly synchronized, except, *perhaps,* for small numbers of units ticked off, it is unquestionable that it is of great practical value both in science and in everyday life that our clocks should be synchronized as exactly as possible. But the synchronization of clocks *presupposes* regularity of them. What is regularity? It is tempting to suppose that a clock is regular if all of the units of time which it ticks off are always the same "length." I believe that this is an unfounded supposition. I do not think we know, or even have good reason to believe, that *any* clock is regular in this sense, for I do not think that such regularity has a literal or factual meaning. Accordingly, I think it is necessary to look elsewhere for the meaning of "regularity" with respect to clocks. It seems to me that the regularity, and hence the "correctness," of a standard clock has a meaning only in terms of practical convenience.

An "ideal" clock would presumably be one which is most reliable in regularity. Mechanical clocks suffer from various defects in this regard. They get dirty, and the tension of their springs varies with temperature and age. These and other factors make them liable to irregularity. It follows from this that a clock which depends least on mechanical repetitions of pattern will be more reliable in regularity, provided it is not subject to other unpredictable disturbances. But what does this regularity of a clock *mean?* Surely, no one would want to assert that regularity has an *intrinsic* meaning apart from some *comparison.*

Suppose we have two clocks, a mechanical one, A, dirty, old, and subject to disturbing influences, and another, B, which has an absolute minimum of mechanical moving parts, clean, and to a high degree free from disturbing influences and the ravages of age. If we compare the performances of A and B, and we find that A often varies in the number of units of time it ticks off compared with B, counting from arbitrarily chosen simultaneous startings to arbitrarily chosen simultaneous stoppings, there is a tendency to say that A is irregular and that B is more correct in the sense that B is more reliably regular. Insofar as we know, or think we know, the reasons why A varies in comparison with B, we can justifiably assert that B is more correct. But unless we suppose that B is *intrinsically* correct, intrinsically regular, in some non-factual sense depending on a supposed equality of the "lengths" of its units, we shall have to admit that its correctness and regularity is merely a matter of *convenience*. Compared with clock A, clock B is more convenient for the performance of physical measurements, and, in general, for the achievement of reasonably simple generalizations concerning physical events. Of course, for some purposes, it may be necessary to calculate what the performance of a fairly reliable standard clock would be if it were free from what, in accordance with our usual understanding of physical laws, we interpret as a disturbing influence. Thus, if the earth is used as a standard clock, corrections may be calculated to allow for the gravitational influence of the moon. Once we have chosen a certain clock as a standard, we can achieve definitions of physical factors which, together with certain accepted physical laws, will enable us to describe the conditions under which the standard clock functions. We can then calculate what the performance of the clock would be if it were not subject to these disturbing conditions.[4] But this is merely an illustration of the point that, as a matter of practical convenience, we prefer to correct our clocks, rather than to correct our understanding of physical laws. Thus, the regularity of our clocks is a function of the simplicity and convenience of our *statements* of certain physical laws. But there is nothing theoretically impossible about understanding nature in terms of exceedingly complicated statements, in which provision has to be made for the variability which we would ordinarily ascribe to an irregular clock.

Thus, it makes very good sense to say that some clocks are more correct than others in terms of comparative regularity,

[4] Cf. Victor F. Lenzen, "Procedures of Empirical Science," in *International Encyclopedia of Unified Science*, Vol. I, Part 1, pp. 295 ff.

and in terms of practical convenience. But it makes much less sense to say that the more ideal a clock is in terms of comparative regularity, the more reason we have for supposing that we know that the units of time which our ideal clock ticks off are always, or nearly always, the same "length." The most ideal clock would be one of which we knew that its recurring pattern was absolutely independent of any external interference, and one of which the repetitions could be "read" by a sort of pure intuition involving no natural events whatever. But even if we had such a clock, there would be no reason to believe that the units of time it ticked off were always the same "length." If we suppose, as I think it absurd to do, that it is permissible to speak of the "length" of a standard unit of time, we would have to admit that without comparing our ideal clock with some other less ideal clock, we would have no means whatever of knowing whether or not some units of time ticked off by our ideal clock were shorter or longer than other units. Apart from some comparison, the question would be literally or factually meaningless. But if we compared the performance of our ideal clock with that of some other clock, we would always have the *choice* of saying that the ideal clock was irregular, and amending our statements of physical laws, or of saying that the other clock was irregular. And as to which of these alternatives should be chosen I can see no theoretical grounds for decision, but only practical grounds of convenience. If we were sufficiently Godlike, we could achieve descriptive generalizations equally as significant as those we now have, no matter how "irregular" our standard clock might be. Since we are not Gods, however, it behooves us to correct our clocks rather than to correct our statements of physical laws.

Of course, it will be objected that in this paper I am illegitimately identifying Time with measured time, or units of time. I have no intention here of entering into a discussion of the "meaning of Time," but it seems to me that anyway the shoe is on the other foot. It is the critic of flowing time who identifies Time with units of time, for it is *he* who says that if time flows, we ought to be able to ask how fast it flows. If this question is seriously meant, it must presuppose that time is composed of units or, what amounts to the same thing, is measurable in terms of units. It is only that time which can be measured that can have a rate of change. If the critic means by "time" something that cannot be measured, then he cannot even ask his crushing question. But if he means by "time" something which can be measured, then his question is capable of being answered literally

with a serious, straightforward answer. Thus, if he asks, "How fast is time flowing?," one need merely have him specify the time of *what* clock, and the rate of change relative to the time of *what* other clock. Then one can answer him. Otherwise, his question is, indeed, unanswerable, but not because time does not flow. It does. At least, the kind of time indicated by his question flows—on some occasions faster, and on others, slower, depending on what clocks are involved.

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BOOK REVIEWS

Pictorial History of Philosophy. DAGOBERT D. RUNES. New York: Philosophical Library [1959]. x, 406 pp. $15.00.

Wisdom of the West. BERTRAND RUSSELL. Editor: Paul Foulkes. Designer: Edward Wright, with ten Compositions by John Piper. Garden City, New York: Doubleday & Company, Inc. [1959]. 320 pp. $12.50.

By one of those coincidences we can demonstrate could not possibly have happened, the same week in November saw the publication in the U. S. of two pictorial histories of philosophy. Mr. Runes's will inevitably suffer, for his annotations to his pictures cannot compete with Russell's sustained narrative. This is a pity, for his collection of illustrations, both older engravings and 19th and 20th-century photographs, is far more comprehensive and interesting than Russell's. It seems to have been assembled with loving personal care. The illustrations to Russell's book have in contrast the slick quality associated with the English equivalent of Madison Avenue. They are mostly in colors, while Runes's are in black and white—admirably suited to engravings and photographs. One of the most interesting features of the Russell book is the facsimiles of the title pages of most of the philosophical classics since the invention of printing, down to Bergson and James.

As in so many duplications of this sort, the upshot is, he who really wants a pictorial history of philosophy will have to procure both. For the two are hardly rivals—they rather supplement each other. He who wants a very popularly written introduction to the history of philosophy, that may well perform for our generation the useful function performed by Will Durant's *Story of*

Philosophy for the last, will get Russell's book. His 300-page text is completely different from that of the earlier *History of Western Philosophy*. Either Russell has learned and profited from the criticisms of the idiosyncrasies of that earlier volume, or he has been wisely guided by his collaborator—probably both. This book is balanced and judicious; it tries seriously to do justice to those philosophers with whom Russell has an imperfect sympathy, like Aristotle, the later Whitehead, and the Existentialists. It even attempts to set forth sympathetically Wittgenstein's linguistic therapy, before delivering its devastating criticism. And it has some penetrating insights into Dewey, though ultimately it gets no nearer an understanding than before.

But it is hoped later to deal here at greater length with Russell's text. At present the emphasis will be upon the pictures. Russell has introductory sections dealing with the cultural currents of each of some ten eras. Illustrating these are not only such things as colored photographs of Cnossos, of Delphi, of a Greek temple, etc., but also ten abstract "compositions" in color by John Piper, whose decorative value is quite successful, but whose function as illustrative of anything is dubious. Moreover, the Russell volume makes full use of many "diagrams" of the visual-aid variety. Some of these are very elementary, like the shadows in the cave for Plato (p. 60), the matter and form diagram (p. 82) or the mean (p. 93) for Aristotle. Some hardly come off, like the potentiality-actuality diagram (p. 83). But some are quite successful, like the rationalism-empiricism contrast (p. 253). On the whole, these diagrams seem more amusing than instructive for serious readers. But they may well serve a purpose for a beginner, or a beginning class.

Russell's book is confined to the West, for, he insists, only in the West is philosophy to be found—only there is wisdom interpenetrated with a tradition of scientific thinking. Indeed, he holds that Western philosophy is really synonymous with the fortunes of Greek philosophy, and over half his material deals with Greek thought. He is disturbed at those contemporary philosophical movements that are willing to cut off their roots in the intellectual tradition. Russell's historical interests are genuine.

Runes includes in philosophy the wisdom of the East as well. He has illustrations not only of the teachers of Hindu and Far Eastern cultures. He starts with a long section on Judaism, from Moses to Buber, in which Spinoza has his place (and not in the 17th century), with many photos of his shrines in Holland. There are "likenesses" of the medieval Jewish and Arabic philosophers.

There are amusing caricatures of the classic Germans. Most interesting of all are the many photographs of 19th and early 20th-century German, French, Italian, and British academic philosophers and psychologists, who for most of us are names rather than images. There is an excellent and comprehensive series of photos of the leading American figures of the last generation, culminating in Einstein and the final picture—a mushrooming cloud labeled $E = mc^2$.

Mr. Runes has his convictions. The series of German philosophers ends with a picture of Heidegger in knee-breeches—Russell has that also—facing a most comical picture of Hitler in a full-dress suit. The caption runs: "Adolf Hitler, the spiritual father of a New Europe, according to an enthusiastic Heidegger." There is a picture of Hitler admiring half a bust of Nietzsche. Opposite Russell's picture is a large photo of Stalin, with the caption:

> Joseph Stalin, the chief architect of Russian imperialism, who by admission of his own lieutenants executed three and a half million Russian peasants and thousands of his personal coworkers.
>
> On frequent occasions Bertrand Russell deviated from his philosophical work into the realm of social idiosyncrasy and anti-American platitudes. He delighted in attacking the United States as a monger of atomic warfare, advocating a general acceptance of Soviet Russian world dominance. [P. 269.]

Amongst the psychologists, Freud receives a note of five lines, while Jung has a column and a half. And there is one slip: on p. 336 the captions for the pictures of Ernst Cassirer and Nicolai Hartmann have been interchanged.

Doubtless both Russell and Runes would agree with Plato, that sense images are not to be identified with philosophical knowledge. Pictures do not carry us far toward philosophical understanding. But after all, philosophy is a human wisdom, and a good portrait can illuminate the man who utters that wisdom. Even with the earlier Greeks, whose portrait busts are generally held not to be likenesses, the type suggests what they came to mean to men. In the same way, the imaginative portrayals of the Eastern and Old Testament figures can tell us much about the human significance of their teaching.

What it boils down to is this. If you want a good brief popular history of philosophy, dealing with the conventional figures, take Russell. You will get with it a slick and sophisticated book of pictures, some half of which have something to do with the philosophers. If you are looking for a collection of interesting iconographical material dealing with the history of philosophy,

take Runes. If you can afford it, take both. Each is well worth the price.

J. H. R., JR.

Unpublished Letters. FRIEDRICH NIETZSCHE. Translated and edited by Kurt F. Leidecker. New York: Philosophical Library [1959]. 156 pp. $3.75.

In Letter number 40 of this collection, Nietzsche refers to "insulting and agonizing experiences" which he had suffered. He might use very much the same terms to describe the present volume. *Unpublished Letters* is a thoroughly misleading title. In his Introduction the editor is more accurate, even if not entirely idiomatic:

> But, owing to Professor Schlechta's edition of the works of Nietzsche in three volumes, with the third volume bringing the literary remains as they were before they were edited under supervision of the sister, and the letters with the text-critical apparatus, we can say now that we have a competent edition which will enable anyone to attain a truer, more clarified conception of Nietzsche's life and thinking.
>
> As to the letters translated here, most of them for the first time, they represent a selection from the 278 letters excellently chosen and published by Schlechta in the third volume just referred to.

If this quotation corrects the inaccuracy of the volume's title it also serves, unfortunately, to illustrate the quality of the editor's work. On page 14 of his Introduction he writes:

> Compare with Nietzsche's style the style of German scholarly productions and the style of books written then as now without regard to the niceties of language. Of course he, like ourselves, would be pained to read them or lay them aside unread, being sensitive to language as to music.

Nietzsche would surely be "pained to read" this rendering of his letters. The "niceties of language" in the Introduction are consistent with the translation of the letters. The following are some samples:

> As for me, however, I shall join not until Easter.
>
> Now, I am also in the rather inconvenient position to have really a great number of interests which are distributed over different fields. . . .
>
> Before joining I became intimately acquainted also with the Marchia and elected some for more intimate intercourse. . . . Until now I have experienced on all hands much that is pleasant and dear.
>
> I received the box and am greatly pleased over it, especially the nice linen things and the beautiful music literature. Yesterday we enjoyed a most jolly afternoon. I danced fabulously.

Please try and picture my situation to you.

Of course, Wagner is in a very pregnant sense beneficial especially as an example incomprehensible on the basis of traditional esthetics.

He is now writing to a relative in Basel that he is thanking God for not having gone to a university where *I* am a teacher. . . . Still one could, at all events, bear that yet.

Sunday a week I shall sojourn to Italy. . . .

One could laugh over this.

One could indeed laugh at this perversion of Professor Schlechta's research and scholarship. But one wonders whether he was consulted and whether he can secure no redress from the Philosophical Library.

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NEW BOOKS

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NOTES AND NEWS

The Virginia Philosophical Association will hold its twentieth annual meeting on October 14 and 15, 1960, at Clinch Valley College, Wise, Virginia. Papers will be read by members of the Association.

An international symposium regarding the works of Nicolò Cusano, organized by the Faculty of Education of the University of Padua, will be held at Bressanone (Bolzano) from July 21st to July 23rd, 1960. The following professors have promised to take part in the meetings: E. Colomer, B. Decker, M. de Gandillac, C. Diano, E. Garin, H. Hallauer, R. Haubst, J. Koch, B. Nardi, E. van de Vyver, and P. Wilpert. Anyone interested in the symposium who desires further information should write to the Segreteria della Facoltà di Magistero, Università di Padova (Italy), or to the Segreteria dei corsi estivi della Università di Padova, Bressanone-Bolzano (Italy).

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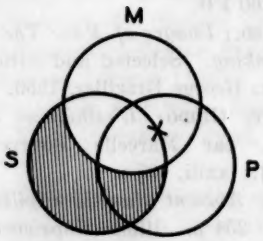

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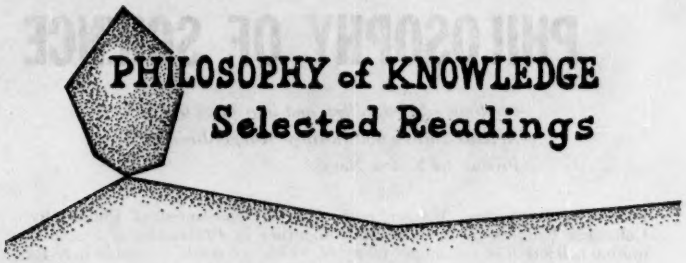

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